pain relief was required earlier in these nine children than in the children who had alternative intraoperative analgesia (n=9) (3·3 h *v* 7·6 h).

A conflict therefore arises over whether we should persist with tonsillar block and improve our performance even though we know that some children will be in pain while we are improving or whether we should abandon it for the method that gives better analgesia in our hands. We would now have difficulty in gaining the approval of an ethical committee for a trial of tonsillar block, so such a trial would not be included in an evidence based database even though it would answer a useful question. We also thought that parents would not consent to a trial investigating a method that we had shown to be inferior. This means that the question of pain relief may not be amenable to investigation with the criteria that have been imposed for evidence based medicine. Many more questions of interest would also be unanswerable.

Side effects of drugs come squarely into this category. Parke *et al*'s report of deaths after administration of propofol in children did not meet the criteria for inclusion in an evidence based database.[2] In our hospital an early death occurred after administration of propofol. We were told that there were so many confounding factors in our case that no inference was possible: Parke *et al*'s report certainly changed our practice. We conclude that some questions cannot be decided by the rigours of evidence based medicine and that, for others, answers are provided only after an unacceptable delay.

We challenge Frank Davidoff and colleagues to apply their criteria for evidence based medicine to the change of practice that they are advocating: is there any good evidence that evidence based practice works? The issue of the *BMJ* containing Davidoff and colleagues' editorial also contains a letter that investigates use of the Cochrane database.[3] A more relevant question, however, is whether the outcome data for doctors who use the Cochrane database are better or worse than the data for those who do not.

Although evidence based medicine is a good way of deciding some issues, it is not the only way forward. If we use it as the only way forward we should recognise that we will not be able to answer some questions for which we will need to know the answers.

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2 Parke TJ, Stevens JE, Rice ASC, Greenaway CL, Bray RJ, Smith PJ, *et al.* Metabolic acidosis and fatal myocardial failure after propofol infusion in children: five case reports. *BMJ* 1992;305: 613-6.
3 Hyde C. Who uses the Cochrane pregnancy and childbirth database? *BMJ* 1995;310:1140-1.

No guidance is provided for situations for which evidence is lacking

EDITOR,—The paradigm of evidence based medicine provides clear guidance for making decisions about the delivery of health care in populations.[1,2] Therefore, it must be qualified so that it can be used for managing specific patients.

Evidence based medicine finds answers only to those questions open to its techniques; randomised controlled trials are its capstone. Other types of evidence, including natural observations and pathophysiological principles, are ranked lower and heavily discounted. Nevertheless, for many rare disorders and an increasing number of subgroups of patients we will never have higher levels of evidence. Evidence based medicine does not provide guidance when one is trying to make sense of situations for which better types of evidence are lacking. Even when we have better evidence it is difficult to apply it to a particular patient. When one treatment is shown to be better than another on a population basis this does not mean that it is the best treatment for the patient. Only when we cannot reliably predict which patient will benefit from each treatment option should we take the general conclusion for the population and apply it to the individual patient.[3] Establishing predictive assays and baseline variables to stratify for risk and benefit may help to match treatment options to subgroups of patients. The evidence for this, however, is likely to be of a lower level, partly because of increasingly small sample sizes.

Furthermore, we present treatment options to a patient to elicit informed consent. Some patients may prefer to receive a treatment that has been classified as less efficacious by evidence based medicine. Some doctors may be willing to give the treatment, and some taxpayers may be willing to pay for it, to empower the patient by offering a meaningful choice. Also, patients have to compare different procedures of techniques and trade off the chances of benefits and of toxicities, the severities of outcomes, and their temporal relationships. Each patient will do this slightly differently and may prefer a treatment option that evidence based medicine and its derivatives (practice guidelines[4] and economic analyses) suggest should be abandoned.

We enjoy using evidence based medicine in our practice and research efforts but believe that it fails to address how individual, public, and social choices are made. It is useful in the design and appraisal of studies, which can provide us with quality data about multiple treatment options. While high quality evidence based medicine definitely informs us, however, it cannot make decisions for our patients.

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Quality cannot always be quantified

EDITOR,—William Rosenberg and Anna Donald examine some of the disadvantages of evidence based medicine but, for each "threat," argue that an overall "opportunity" to improve existing practice ensues (with the implication that the debate is more polarised than perhaps it is).[1] One important disadvantage is not addressed, though it is encapsulated in another context earlier in the article where the effectiveness of evidence based medicine is considered: "the approach is difficult to evaluate . . . since many [outcomes] are difficult to quantify."

Surely this is a problem with evidence based medicine. Most outcomes of health care are multifactorial, and it is mainly those outcomes that lend themselves to direct quantitative assessment that are used in randomised controlled trials—measuring the measurable. To extrapolate from the example given in the article, it is important to consider all the effects on a patient of initiating a lifelong course of warfarin; these include the inconvenience of daily treatment and regular monitoring and review; disruption of existing and future drug treatment because of drug interactions; and potential psychological morbidity resulting from imposition of the sick role by treatment of an asymptomatic condition. These outcomes cannot be measured, but the risk of haemorrhage and reduction in the risk of stroke can be, so these are the figures on which decisions are to be based, argue the authors.

Where gaps in evidence are likely to exist we are exhorted to surmount the problem by attempting to answer only questions "for which there is likely to be good evidence" and awaiting the results of future research. There are no suggestions on how our practice relating to unmeasurable aspects is to be guided.

An analogy might be where hospital managers are tempted to highlight waiting time for patients at outpatient clinics as an indicator of the clinics' performance. As doctors and patients we believe that the content of the eventual consultation is more important than the waiting time, but the measurement is easy and relatively precise. Neither of the implicit assumptions—that patients' satisfaction equates with their waiting time and that quality of health care given equates with patients' satisfaction—can be made.

The principle of critical appraisal is laudable in all branches of higher education and professional practice and may have been deficient in medical education and practice. The wholesale adoption of evidence based medicine, given the current limitations of comprehensive outcome measures, is premature. A gradual and integrated introduction may be more appropriate.

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Journal of Evaluation in Clinical Practice will start publication in September

EDITOR,—The "Editor's choice" in the issue of 29 April welcomes the publication of the new journal, *Evidence Based Medicine*, which is described more fully in Frank Davidoff and colleagues' editorial.[1] In addition, Aneez Esmail reviews a new textbook entitled *Evidence-based General Practice*.[2] These are important contributions to the inexorable development of evidence based health care.

I wish to draw attention to a new clinical journal that is similarly dedicated to evidence based medicine but within the broader context of multidisciplinary clinical care and analysis. The *Journal of Evaluation in Clinical Practice*, whose first edition will be published in September, will focus on systematic reviews of research on clinical effectiveness; the implementation of evidence based care in routine clinical practice; systematic medical and clinical audit; methodologies for quantifying clinical benefit from audit and after the implementation of evidence based care; systematic reviews of the current status of audit and its development in medicine, nursing, midwifery, and the range of professions allied to medicine; and the functional integration of clinical audit, research and development, and quality assurance in health care organisations.

A textbook focusing on such central issues in modern medicine, *Effective Clinical Practice*, will be published in November[3] and is concerned with clinical effectiveness, clinical appropriateness, the efficiency of the delivery of clinical services, the

methods by which changes in these variables can be measured, and the authority and legal standing of guidelines on clinical practice.

All of these initiatives will have a pivotal role in accelerating the development of modern clinical care and in replacing clinical opinion with clinical evidence.

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1 Davidoff F, Haynes B, Sackett D, Smith R. Evidence based medicine. *BMJ* 1995;310:1085-6. (29 April.)
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Accurate references are important

EDITOR,—William Rosenberg and Anna Donald praise the concept of evidence based medicine, but in their example they ignore the evidence.[1] They say, "It was decided to set a target international normalised ratio of 1·5-2·0." This is based on four references, three of which I have checked to find that they aimed at ratios of 2·8-4·2,[2] 1·4-2·8,[3] and 2-4·5.[4] Rosenberg and Donald do not provide the evidence supporting their choice of a lower ratio.

The search for evidence is helped by accurate references: two of these three are incorrect.

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Authors' reply

EDITOR,—We are pleased that David A Fitzmaurice admires our efforts. By forcing us to be explicit, the practice of evidence based medicine often identifies issues over which reasonable doctors can disagree, such as the therapeutic range. The fourth step of evidence based medicine, "acting on the evidence," involves clinical freedom and encourages the appropriate use of clinical responsibility, specifically to use the best available evidence when taking decisions about individual patients that consider all aspects of their care. Evidence based medicine does not force compliance with guidelines produced externally. With regard to an elderly woman at increased risk of haemorrhage, we disagree with Fitzmaurice's recommendation. The British Society of Haematology's guidelines are based on an earlier review,[1] which amplifies the flaws in evidence available at the time of their use. The more recent trials cited in our article provide far more reliable evidence of benefit from anticoagulant treatment, with international normalised ratios in the range 1·4-4·5. We believe the lower range to be safer for the patient, and our local anticoagulation laboratory recommends the narrow target range of 1·5-2·0.

Bruce G Charlton provides us with the opportunity of correcting the common misunderstanding that evidence based medicine is merely the mindless application of the results of megatrials. Evidence based medicine uses many forms of evidence, some of which come from the basic medical sciences and some from large trials (surely a form of medical science). Trials (of whatever size is appropriate to the question they address) are unparalleled in providing estimates of efficacy, but in acting on the evidence thoughtful clinicians add their own estimates of susceptibility and, most important, patient utility to individualise the evidence. Charlton's accusations of non-randomness and selection bias are equally applicable to the case series of experienced clinicians and randomised controlled trials.

Malcolm Griffiths's letter allows us to re-emphasise the necessity to update systematic reviews of health care periodically. This is emphasised in the opening page of the reference he cites as well as its successor, the *Cochrane Database of Systematic Reviews.* We urge Griffiths to accept the database's invitation to identify additional evidence and suggest additional reviews for the update now under way.

In answer to Oliver Dearlove and colleagues, when we consider patients rather than procedures, over half the patients treated in a busy general medical service in Britain receive interventions that are based on randomised controlled trials or meta-analyses and an additional third receive interventions whose value is self evident.[2] Two other studies show the efficacy of teaching evidence based medicine, one of which we cited in our article.[3,4]

Glenn W Jones and Stephen M Sagar have a narrow view of evidence based medicine. We strongly disagree with their assertion that randomised controlled tirals provide the only form of evidence to be used in evidence based medicine, nor do we believe that other forms of evidence are heavily discounted by evidence based medicine. As pragmatic clinicians we aim to use the best available evidence, of whatever type of source. Jones and Sagar overlook two fundamental steps in evidence based medicine: critical appraisal and the application of evidence to clinical practice. Critical appraisal of evidence does not value randomised controlled trials while rejecting all other forms of evidence. Well conducted randomised controlled trials generate valuable, high quality evidence for some questions but not others, and in some areas the results of randomised controlled trials are not available. In either situation we must use other types of critically appraised evidence when forming clinical decisions.

Jones and Sagar suggest that the practice of evidence based medicine necessitates blind application of the results of large trials; this is a common misconception. Patients' preferences and their individual characteristics, as well as doctors' preferences, play a part in the fourth step in evidence based medicine—applying the evidence to clinical practice. Jones and Sagar state that evidence based medicine "cannot make decisions for us." We advocate freedom to make clinical decisions informed by the best available evidence and regard that as the practice of evidence based medicine.

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Compliance with medication among elderly people

Study of self medication on elderly people was flawed

EDITOR,—Self medication programmes and compliance among elderly people are important.[1] Caution needs to be exercised, however, before we accept Catherine Lowe and colleagues' conclusion that a self medication programme improves compliance. The patients clearly knew which group they belonged to in the experiment; it is unclear whether investigator EAC (who followed up the patients) was blind to the patients' experimental status. Absence of blindness coupled with use of an unvalidated interview may have biased the results. For patients taking more than one tablet, a mean compliance score was calculated; these mean scores were later used in the statistical analysis to show a difference between the two groups. As the median number of drugs received by patients was four, the use of mean scores may be statistically inexact and inaccurately assume a normal distribution.

Compliance was high in both groups. This is hardly surprising as patients likely to comply poorly were excluded from the experiment at the outset. It would have been interesting if such patients had been included.

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1 Lowe CJ, Raynor DK, Courtney EA, Purvis J, Teale C. Effects of self medication programme on knowledge of drugs and compliance with treatment in elderly patients. *BMJ* 1995;310: 1229-31. (13 May.)

Author's reply

EDITOR,—A K Shah raises several points about our study. Firstly, the investigator (EAC) was not blind to the patient groups but collected data with a standard questionnaire to remove bias. Secondly, median numbers of medicines were quoted in table I as this was thought to be more appropriate than describing patients as taking fractions of tablets. Thirdly, compliance scores were compared with the Mann-Whitney U test, which is valid for non-normal distributions. This test ranked the mean compliance scores for each patient and did not compare means for the two groups. Finally, as stated and discussed in the paper, we excluded only patients not responsible for taking their medicine and terminally ill patients, who were clearly not appropriate for the study.

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Prophylactic aspirin and peptic ulcer bleeding

Patients warned of a bleed may be more vigilant

EDITOR,—John Weil and colleagues, in their large case-control study, do not mention that they have taken the possibility that their aspirin using subjects could have been more likely to spot a bleeding peptic ulcer because they would have been warned of the risk, and may therefore have had a lower threshold for detecting gastrointestinal haemorrhage.[1] If this factor was a material consideration then the extra bleeds detected and reported would have been of small size, and would presumably have been melaenas rather than haematemeses.

A minimum of 50 ml of blood is required to